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Address delivered by the President, M. J. Johnson, Esq., on presenting the Medal of the Society to M. Schwabe:—

Gentlemen,—The order of our proceedings has now brought us to the presentation of the medal which, as you have heard, the Council has awarded to M. Schwabe, for his discovery of the periodicity of the solar spots.

M. Heinrich Schwabe is a gentleman resident in, and I believe a native of, Dessau, the capital of the Duchy of Anhalt Dessau, who, having no professional duties, has devoted much time and attention to scientific studies, and has attained considerable reputation in other departments of knowledge than that in which he is known to this Society. Among other accomplishments, I am informed that M. Schwabe is an excellent botanist, and has composed, though, I believe, not published, a flora of the neighbourhood of Dessau.

It was in 1826 that he entered upon those researches which are now to engage our attention. I am not aware of the motive that induced him—whether any particular views had suggested themselves to his own mind—or whether it was a general desire of investigating, more thoroughly than his predecessors had done, the laws of a remarkable phenomenon, which it had long been the fashion to neglect. He could hardly have anticipated the kind of result at which he has arrived; at the same time we cannot imagine a course of proceeding better calculated for its detection, even if his mind had been prepared for it, than that which he has pursued from the very commencement of his career. Assuredly if he entertained such an idea, it was not borrowed from the

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authorities of the last century, to whom the solar spots were objects of more attention than they have been of late years.

"Nulla constanti temporum lege apparent aut evanescunt," says Keill in 1739.—*Introduct. ad Physic. Astronom.* p. 253.

"Il est manifest par ce que nous venons de rapporter qu'il n'y a point de règle certaine de leur formation, ni de leur nombre et de leur figure," says Cassini II. in 1740.—*Elém. d'Astron.* vol. i. p. 82.

"Il semble qu'elles ne suivent aucune loi dans leur apparitions," says Le Monnier in 1746.—*Instit. Astron.* p. 83.

"Solar spots observe no regularity in their shape, magnitude, number, or in the time of their appearance or continuance," says Long in 1764.—*Astron.* vol. ii. p. 472.

"Les apparitions des tâches du soleil n'ont rien de regulier," says Lalande in 1771.—*Astron.* vol. iii. § 3131, 2d edit.

And Delambre's opinion may be inferred from a well-known passage in the third volume of his *Astronomy* (p. 20), published in 1814, where treating of the solar spots he says, "Il est vrai qu'elles sont plus curieuses que vraiment utiles."

I cite these passages to show, whatever might have been Schwabe's own view of the matter, that a periodicity of the spots was not a current idea at the time he entered upon the subject, and therefore that he has not merely developed a law for which men's minds were prepared, but that he has been, to all intents and purposes, the discoverer of it. The first suggestion I have met with (and that a very vague one) of anything of the kind occurs in the article "Sonnenflecken," in Gehler's *Physicalisches Wörterbuch*, written by the elder Littrow, and published in 1836, long after Schwabe had been at work.

It was in 1826, as I have said, that Schwabe began his observations, but I am not aware of any published results before those in No. 350 of the *Astronomische Nachrichten*, which appeared in April 1838. Here he gives a summary of twelve years' labour, and though at this time he must have begun to foresee the issue, for the indications of periodicity are plain, he makes no remark, nor does the memoir appear to have attracted the attention of astronomers.

From this time commence his annual contributions to the *Astronomische Nachrichten;* but it was not till the end of 1843, when he had passed through two periods of maximum and minimum, that he very modestly remarks that his observations heretofore had given indications of periodicity which that year's result tended to confirm. Still the subject attracted little attention. As far as I can discover, the only person who had taken it up was Julius Schmidt, the present indefatigable Director of the Observatory at Olmütz, then residing near Hamburg. But the philosopher of Dessau was not disconcerted; he went on accumulating fresh proofs of his great discovery, which, when announced in 1851, by Alexander Von Humboldt in the third volume of his immortal *Cosmos*, came upon the world with all the freshness of

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novelty, though in reality the secret had been revealed eight years before.

Now, gentlemen, what were the means and processes by which Schwabe arrived at his results? These he tells us in No. 350 of the *Astronomische Nachrichten*, to which I have already referred.

His instruments are two telescopes, by Frauenhofer, one a 3½, the other a 6-foot, with powers of 45, 64, and 96, the last only used in extreme cases. The apertures of his object-glasses were generally reduced to 1¾-inch and 2½-inches, an arrangement by means of which he obtained the double advantage of being able to protect his coloured glasses, and to use lighter tints than would have been otherwise practicable. He particularly recommends glasses of a certain blue tint which he obtained from Munich.

His observatory, I am informed by Mr. Carrington, is a small apartment on the top of his dwelling-house.

His method of observation is as simple as his apparatus, and he has felt throughout his inquiries the force of the precept, as necessary in cases of this kind as in that which the poet had especially in view,—

> "Servetur ad imum,
> Qualis ab incepto processerit, et sibi constet;"

from first to last he has never altered his system of classification and registration.

His plan is to note by a number each spot in the order of its appearance, carrying on his notation from the first to the last spot in each year. He reckons an isolated spot, or a cluster of spots where there is no visible separation between their penumbræ, as one group. "Hence," he observes, "the number of *spots* will depend in a great measure on the excellence of the telescope; and it often happens that clusters of many hundred, nay of many thousand spots, will be designated by one number only, just as a single isolated spot will be. So great, however, is the sun's tendency to present his spots in the form of clusters, that other observers will, in the course of a year, assuredly not find any great difference between their numbers and mine." But he particularly impresses on his readers that he attaches importance not so much on the absolute number of the groups, as on the ratio which obtains between them in different years.

The result of his investigations, as you are aware, has been to establish with a degree of probability almost amounting to certainty, that the solar spots pass through the phases of maximum and minimum frequency, and *vice versâ*, in a period not very different from ten years.

The exact period Schwabe does not pretend to have determined. That it is liable to perturbation is evident. During twenty-seven years of the series the results were extremely regular; during the last three years they have shown symptoms of disturbance. The epoch of minimum, which, consistently with earlier indications, should have happened in 1853, did not occur until 1856.

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Such is the brief history of thirty years' labour, which, had I wished, I could hardly have prolonged: in fact, descriptively, there is little more to say about it.

You will have observed that the instruments were of the simplest kind. And though the arrangements in the first instance, as must be the case in all physical researches, called for the exercise of thought and intelligence—qualities the possession of which, in a very high degree, no one who reads M. Schwabe's papers in the *Astronomische Nachrichten* will deny him—these arrangements once made, the observations were of a description requiring for their prosecution more experience than skill.

It was not then for them, or for any special difficulty attending the research, that your Council has thought fit to confer on M. Schwabe this highest tribute of the Society's applause. What they wish most emphatically to express is their admiration of the indomitable zeal and untiring energy which he has displayed in bringing that research to a successful issue.

Twelve years, as I have said, he spent to satisfy himself—six more years to satisfy, and still thirteen more to convince, mankind. For thirty years never has the sun exhibited his disk above the horizon of Dessau without being confronted by Schwabe's imperturbable telescope, and that appears to have happened on an average about 300 days a-year. So, supposing that he observed but once a-day, he has made 9000 observations, in the course of which he discovered about 4700 groups. This is, I believe, an instance of devoted persistence (if the word were not equivocal, I should say, pertinacity) unsurpassed in the annals of astronomy. The energy of one man has revealed a phenomenon that had eluded even the suspicion of astronomers for 200 years!

Let us hope that the example will not be lost. Men are apt to speak of astronomy as an exhausted science, meaning that all that can be known is known. No doubt being the most perfect, it is in one sense the most exhausted science. But the astronomer of Dessau has taught us that there are still mines rich in ore, though they lie deep buried, and must be worked with more assiduity and with more care. I can conceive few more unpromising subjects, from which to extract a definite result, than were the solar spots when Schwabe first attacked them.

The only recent instance of perseverance, I can call to mind, comparable with the one before us, is that of Hencke, of Driessen, who is said to have spent the leisure of fifteen years in search of another planet; and at last, by the discovery of *Astræa*, gave the impulse which, responded to by Mr. Hind, De Gasparis, and others, has numerically increased more than four-fold the known members of the solar system. The achievement in either case may not fall within the category of works of genius—but both resulted from qualities of mind scarcely less rare than genius, and are precisely of a kind that genius alone never could accomplish.

The observations of Julius Schmidt, and Professor Wolf, lately

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of Berne, now, I believe, of Zurich, who has regularly observed the spots since 1849, fully confirm the general accuracy of Schwabe's results. But we are naturally led to inquire whether we can connect them with the statements of former observers. Professor Wolf, who has entered deeply into the general question, published a few years ago, in the *Memoirs of the Philosophical Society of Berne*, all the notices he was able to find, scattered in books and memoirs, from the time of the discovery of the spots in 1611; and from a comparison of them with more modern determinations he deduced a period 11·1 years. Without depreciating the industry and ingenuity displayed in that paper, I confess I am not disposed to place much reliance on the result, owing to the insufficient data on which it appears to me to be founded. The inquiry is rendered very difficult, because the old observers attended much more to the physical characteristics of the spots, and to their time of rotation, than to their number. Moreover, I know only of Scheiner and Derham who observed them continuously for any length of time—that is to say, for more than two or three years. Scheiner's attention was almost exclusively directed to physical peculiarities; and Derham's investigations (*Phil. Trans.* 1711) were obviously very desultory.

Schwabe has not entered into speculations relative to the nature and origin of the spots, though he has been careful to note all remarkable appearances as they occurred; and of these he has given an admirable summary in the *Astronomische Nachrichten*, No. 473. There he calls attention to an appearance which, he says, is not uncommon, but which he cannot explain on the generally received theory, that the spots are portions of the surface of a solid body, seen through openings in a luminous atmosphere that surrounds it at a distance, and another intervening atmosphere. This theory of Sir W. Herschel, as you are aware, has been found adequate to explain most of the phenomena of the spots. But the case to which Schwabe alludes is this. On the above hypothesis a spot surrounded by a penumbra, will, by the effect of perspective, when it first makes its appearance on the disk, seem to be excentrically situated on the penumbra, the border of the penumbra towards the sun's centre appearing less broad than the other border. All this is intelligible,—but why is not the penumbra equally illuminated all round? For it frequently happens that the border turned towards the sun's centre is dark grey, while that towards the sun's limb is bright grey, and between the latter and the nucleus there is a string of light almost as bright as the sun's disk.

He also mentions having seen, though rarely, the phenomenon which, I find in Arago's notice of the life and works of Sir William Herschel, furnished Francis Wollaston, and Lalande, with an argument against Alexander Wilson, who was the first to advance the theory of the spots being cavities. The phenomenon is this:—Sometimes a spot surrounded by a penumbra passes over

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the sun's disk without the former undergoing any change of relative position, from the beginning to the end of its course. This apparently militates against the cavity theory. Arago says the objection is not insurmountable. "Suppose," says he, "in such cases, that the sides of the openings in the photosphere, through which the spot appears, are not sloping." But according to Wilson's theory the penumbra is *formed* by the sloping sides of the photosphere—and there *is* the penumbra. Therefore it appears to me that this explanation does not hold good on Wilson's view. According to Sir W. Herschel another stratum is interposed between the solid body of the sun and the photosphere. On this hypothesis, it is the part we see of the secondary stratum which constitutes the penumbra; if therefore it be sensibly depressed below the surface of the photosphere a change of relative position must ensue. Schwabe's explanation is, that when the phenomena in question occur the secondary stratum has risen to an unusual height, and is *not* sensibly depressed below the photosphere.

There is another appearance, on which he lays much stress, "Lichtflocken," (light flakes,) which he conceives to be particles of matter, floating on the verge of our atmosphere. But I must not exhaust your patience by entering into details, and refer those who are interested in the subject to Schwabe's own excellent description in No. 350 of the *Astronomische Nachrichten.*

There is, however, one more topic too closely connected with our subject, and too remarkable to pass unnoticed.

You are aware, gentlemen, it has been long known that the intensity of magnetic declination is subject to a daily variation; and this variation was also known to be in some way connected with the sun, attaining its maximum western limit when that body is at its upper and lower culmination, and its maximum eastern limit about 6 o'clock in the morning and evening. These are not the exact times, but they are sufficiently near for our purpose. About the year 1850, Professor Lamont announced that this variation was again liable to another variation, which observed a period, from maximum to minimum, and from minimum to maximum, of about 10 years. He was led to this result by a discussion of his own observations at Munich. It was shortly after fully confirmed by our colleague, General Sabine, by the observations made at the Magnetic Observatories of Toronto and Hobarton. But in the course of the latter discussions General Sabine detected another important circumstance which had escaped Lamont, viz., that the periods of maximum variation of the declination-needle corresponded exactly with those of the maximum frequency of the solar spots, and *vice versâ,* the minimum of the one with the minimum of the other. Hardly had General Sabine's paper been read before the Royal Society on March 18, 1852, when intelligence was received that two Swiss physicists, Professor Gautier of Geneva, and Professor Wolf of Berne, had arrived at the same conclusion, independently of each other, from a perusal of Lamont's results.

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All the observations I have mentioned, were made during nearly the same time—between the years 1840 and 1851—comprehending only a single period of change. Obviously, therefore, in this state of the inquiry it was of the highest importance to obtain a trustworthy series made at another time and under different circumstances. Though hardly to be expected, it happened, most fortunately, that such a series was at hand.

The diurnal variation was one of those subjects which, many years ago, had particularly interested the vigorous mind of the lamented Arago; and among his papers were found the records of a most laborious course of experiments, conducted with all the care which no one knew better than he how to bestow on a delicate investigation. These experiments, extending from 1820 to 1831, have been rigorously discussed by M. Thoman, and exhibit the prevalence of the same law that obtains in the later series.

Nor does the evidence of connexion rest here. In a recent paper sent to the Royal Society, General Sabine has shown that at Toronto (the only series hitherto fully discussed) all the magnetic elements subjected to observation undergo similar variations. In the face of these coincidences, to doubt the relationship between the two phenomena seems to me to be almost as unreasonable as to doubt the influence of the moon on the tides of the ocean.

M. Schwabe's service has been the discovery of an important astronomical fact, falling strictly within the cognisance of this Society. His merits are in no way affected, though, no doubt, the value of his discovery is enhanced, by this remarkable episode. No longer is its scope confined to the disclosure of a physical peculiarity in the constitution of the sun. It promises to be the means of revealing the prevalence of a principle, throughout the solar system, co-extensive with gravitation, and of establishing another link in the chain of analogies binding Earth with other worlds, the ultimate effect of which on the progress of physical research no one can foresee.

The President, then delivering the Medal to Mr. Carrington, addressed him in the following terms:—

I request you will transmit this medal to Mr. Schwabe with the expression of our highest admiration of the ability and zeal with which he has conducted his laborious series of observations on the solar spots—with our hearty congratulations on the results which he has already obtained, and on the bright prospect now opening of their augmented importance. Add, also, our best wishes that his life may be long spared to continue, and to see continued, the course of observations which he has so gloriously commenced.